of other countries, in many ways worse off than our own. The subject is well worth the attention of all who have at heart the higher education of the people.

W. BOYD DAWKINS

THE CARBONIFEROUS FLORA OF CENTRAL FRANCE

Flore Carbonifère du Département de la Loire et du Centre de la France. Par Cyrille Grand' Eury, Ingénieur à St. Étienne. (Imprimerie Nationale, Paris.)

THIS work consists of three quarto volumes, the first of which is devoted to the plants, the second to the geology of the districts under consideration, and the third forms an atlas with thirty-four plates of fossil plants and four large "tableaux," in which the author has "restored" the plants he has described according to his own ideas of their morphology.

It is very obvious that the carboniferous plants of one district cannot be received as altogether typical of those occurring at other and distant localities. Hence such publications as those of Dr. Dawson and Prof. Newbury in America, and the volumes of M. Grand' Eury, are extremely valuable to the English palæo-botanists. They tend to preserve him from the one-sided habit of viewing the subject which he is apt to contract when only studying the types occurring in his own coal-fields. But apart from this M. Grand' Eury's work has an independent value, especially in some departments in which he has made important additions to our stock of knowledge. This is especially the case with his investigations amongst the hitherto obscure plants known as Flabellariæ and Cordaites, as well as amongst some remarkable sporange-bearing ferns.

Our knowledge of Cordaites has hitherto been most vague; but M. Grand' Eury has fortunately obtained some beautiful specimens in which not only the leaves are attached to the stems of several species, but in some he also finds what he believes, I think justly, to be male and female organs of reproduction, thus establishing the point that these plants were monœcious Phanerogams. These organs are slender spikes, some of which support small scaly buds lodged in the axils of bracts, and which the author believes to have been antheriferous. Others bear single seeds in each axil. Some of the spikes are affirmed to be those of Antholithes, and the seeds to be identical with Cardiocarpus. The stems which bear these reproductive structures have a Sternbergian pith, surrounded by an exogenous woody zone inclosed within a distinct bark, which latter appears to have consisted of more than one layer. M. Grand' Eury concludes that these plants were Conifers, of which the well-known Dadoxylons were the ligneous axes, and that the type which survived for a time in some of the Ulmanniæ of the Lechstein, and in the Albertia of the Triassic rocks. I see nothing, however, in his figures and descriptions leading me to conclude that they are identical with our British Dadoxylous.

The new ferns described by the author are equally remarkable. They include numerous forms of Pecopteris, with very peculiar sori approaching those of the Marattiaceæ. Some of these fronds he associates unhesitatingly with Psaronius and other stems of tree-ferns. The author's researches on the above subjects have been conducted under most favourable conditions, of which he has availed himself in a praiseworthy manner.

When we come to the debateable subjects of Calamites, Lepidodendron, Sigillariæ, and Asterophyllites I am obliged to use different language. On these points the author adopts substantially the ideas of Brongniart. Thus he distinguishes between Calamites and Calamodendron, making the former an equisetaceous plant and the latter a gymnospermous one. I cannot understand how any one can do this in the face of our present knowledge of the facts.

In external form the supposed Calamites and Calamodendra exhibit precisely the same appearances. All these appearances are explained in the most exact manner by the internal structure of the many illustrative specimens which we now possess, and which demonstrate that we only have one type of organisation. Further, what are called Calamites by the school to which our author belongs are amongst the most abundant of the plants furnished by our coal-shales, and there is nothing to prevent their being equally common in the Oldham and other beds, in which all the plants retain their internal structure, if they existed as an independent type. But the moment we find a Calamitean plant with organisation it proves to be a Calamodendron. Even M. Grand' Eury is compelled to admit "il est au moins surprenant que l'on n'ait pas mis la main sur un Calamite avec la structure conservée." Very surprising, indeed, considering that we have obtained such numbers of these plants with structure from Oldham, Halifax, and Autun, as well as, though less abundantly, from Burntisland. The conclusion to be drawn is too obvious to need reiteration.

Imbued with these ideas respecting Calamites and Calamodendra, it was inevitable that M. Grand' Eury should fall into error respecting Asterophyllites. These plants are regarded by his school as the branches and leaves of Calamites. Hence he could not recognise as Asterophyllites any plant which had not a Calamitean axis. But I have shown that Asterophyllites has *not* such a structure, but one identical with the very different one of Sphenophyllum. M. Grand' Eury escapes the difficulty by contending that my plants are *not* Asterophyllites, but Sphenophylla. This is certainly not the case. Brongniart has clearly defined the latter genus a possessing 6–8 or 10 *truncate cuneiform* leaves; and after referring to the fructification of Sphenophyllum, he correctly says:—"Ce mode de fructification, malgré l'obscurité qui environne encore sa vraie structure, est trop analogue à celui des Astérophyllites pour qu'on puisse douter de l'affinité de ces deux genres." This conclusion is precisely identical with mine. Instead of 6–8 or 10 leaves in each verticil, my plants have 18 or 20. These leaves are linear, not cuneiform; and as my next memoir will demonstrate even more clearly than I have yet done, each leaf had a single central vascular bundle instead of the two or more invariably seen in Sphenophyllum.

In his views respecting the relations of Sigillaria and Lepidodendron, M. Grand' Eury also clings to the old Brongniartian ideas promulgated in bygone years. M. Brongniart and M. Renault have described the organisation of two Sigillarian fragments, *S. elegans* and *S.*

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spinulosa, and have concluded that whilst their Diploxyloid organisation differs from that of the Lepidodendra, it justifies the conclusion that Sigillariæ were not lycopodiaceous but gymnospermous plants. But I have already shown that several indisputable Lepidodendra have precisely the same organisation. Hence I contend that Brongniart's reasons for separating these plants have no existence, and consequently his conclusions must be abandoned; M. Grand' Eury, forgetting this part of my work, and only remembering that I have also described the bark of a true Syringodendroid Sigillaria, and shown that it is identical in every feature with the corresponding tissue in Lepidodendra, says that I have arrived at my conclusion "par des faits isolés d'après l'analogue de l'écorce, et non par des exemples complets réunissant les caractères extérieurs aux caractères intérieurs." How, in the face of my published memoirs, my friend could make so erroneous a statement, I am at a loss to conceive.

I should have felt it necessary to have subjected the volumes under consideration to an incisive criticism on these and some similar points, were it not that he kindly allows me to quote from some letters which I have received from him. In these communications he says:—"Les points sur lesquels nous différons sont précisément ceux que je n'ai pas étudiés." Referring to facts which I have observed, he adds: "Comme je n'avais pas ces faits pour me guider, j'ai conclu d'après ce que je connaissais bien, et je ne suis pas fain de conclure que je me suis trompé; dans ce cas nous aurions dans les Sigillaires et les Lepidodendrons des cryptogames excessivement elevées en organisation; si elevées qu'ils formaient, en quelque façon, une classe intermédiare entre ces plantes et les Gymnospermes." "L'Association presque constante des macrospores avec le débris des Sigillaires est en faveur de vos conclusions." Quite in accordance with the above remarks are the following observations which the author makes in his volumes: "Il est au moins curieux que, à part le corps vasculaire, les autres parties des Sigillaires soient semblables aux parties correspondantes des Lepidodendrons." This is perfectly true with the exception that the vascular portions are less exceptional than M. Grand' Eury's remarks imply. The Sigillarian stem is merely that of Lepidodendron Harcourtii, *with an additional* exogenous vascular zone interposed between that of the Lepidodendron and its investing cortex; *and which I find in other true Lepidodendra.* If all the plants of the coal-measures which possess a similar exogenous zone are to be transferred from the cryptogamic to the phanerogamic group, there will be few cryptogams left in the carboniferous rocks beyond Lepidodendron Harcourtii and the ferns. M. Grand' Eury concludes his notice of the Sigillariæ by a remark which I fully endorse: "Seulement je crains d'avoir tiré des conséquences trop complètes de ces indices insuffisants, dont je n'aurais peut-être alors même dû parler que pour éveiller l'attention des observateurs sur une solution possible du plus important problème de la paléontologie végétale."

Having thus indicated some very important points respecting which I am compelled to differ from M. Grand' Eury, I can with the sincerest truth again express my sense of the value of this new contribution to the study of the carboniferous flora, and of the praiseworthy perseverance with which the author has laboured for many years in collecting his materials. The most prominent fact which the work reveals is the remarkable abundance of the Cordaites in the coal-measures of Central France, compared with what we see in England. In some districts, as M. Grand' Eury informs us, the coal is almost entirely composed of their *débris*. I have met with nothing like this in Great Britain, but it is in strict accordance with what we know of the distribution of living plants, that whilst similar types may be expected to be met with over wide geographical areas, some forms will predominate in one region, whilst in other localities different types will prevail; hence the materials out of which coal has been found must have been widely different at these various spots.

The plates with which the above work is illustrated are extremely beautiful, as is usually the case with the productions of the French lithographers.

W. C. WILLIAMSON

OUR BOOK SHELF

Annæs do Observatorio do Infante D. Luiz. Magnetismo Terrestre. Lisboa, 1876.

THIS part of the *Annals* of the Lisbon Observatory is a continuation of those noticed in NATURE, vol. xiii. p. 301. The results for the magnetic declination are carried forward from 1867 to 1871, while some include the means from 1858 to 1875. This is the case for the secular change and annual variation. Mr. Capello found previously that the north end of the declination magnet approached the north at the rate of 5′·91 yearly (1858-1868). The results he now divides into two series, 1858 to 1866, with a rate of 5′·46, and 1866 to 1875, with a rate of 7′·64 yearly.

The yearly means are deduced from observations at 8 A.M. and 2 P.M. Mr. Capello has also shown that the diurnal law of disturbance appears to be different at Lisbon in different years of the decennial period.[1] In this case, even if two observations daily were otherwise sufficient to give accurate means, or means strictly comparable from year to year, the varying effect of the disturbance on the observations at the two hours mentioned would of itself interfere with this comparability. It is probably for these reasons that the yearly means at Lisbon do not appear to show the small decennial inequality in the secular movement first indicated by me in 1857, and afterwards discovered by Hansteen and Lloyd.

Mr. Capello has repeated discussions for the magnetic disturbances with the increased materials in his possession. He had observed in a preceding number of the *Annals*, that many observations which were considered disturbed (that is to say, which differed from the means for the hours by 2′·26 or more) really belonged to diurnal variations which were regular, only larger than usual; and it was pointed out in NATURE (in the notice cited above) that one cause of these excessive deviations would be found in the superposed lunar actions. Mr. Capello now finds that a great majority of these quasi-solar disturbances are rather to be considered due to the moon. This conclusion induces me to believe that if Mr. Capello had the necessary aid to perform the calculations for the lunar diurnal variations for each month, and for different positions of the moon, as well as for other investigations, the Lisbon observations could not fail to add many important scientific results to those already published.

JOHN ALLAN BROUN

Incidents in the Biography of Dust. By H. P. Malet. (London: Trübner and Co.)

THE first impression one gets of this book is that of a

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